SPECIAL REVIEW

Animal Species and Evolution. By Ernst Mayr. Belknap Press of Harvard University Press, Cambridge, Massachusetts, 1963: $6\frac{1}{4} \times 9\frac{1}{4}$ in., xiv + 797 pp., numerous figures and tables. $11.95.

Reviewed by Kenneth C. Parkes

Prior to 1942 the name of Ernst Mayr was relatively little known to biologists other than ornithologists. The latter knew him as the young German who had been brought to New York in 1932 to curate the Whitney-Rothschild collection of birds at the American Museum of Natural History. His major field experience had been in New Guinea and the Solomon Islands, and he was an acknowledged authority on the taxonomy and distribution of the birds of the Pacific Islands. Although most of his publications had been faunal or taxonomic, he had written a few thoughtful papers of a more analytical nature, on speciation and zoogeography (Mayr, 1940, 1941). But the name Ernst Mayr could hardly be said to have been a "household word" among biologists.

This situation changed abruptly in 1942 with the publication by the Columbia University Press of Mayr's "Systematics and the Origin of Species." It immediately became apparent that Ernst Mayr was something more than a mere traditional taxonomist. Reviewers rightly praised Dr. Mayr's ability to draw significant generalizations from his own taxonomic work and that of others; particularly did his command of the literature receive admiring comment. "Systematics" soon took its place among the "classical" works of the emerging synthetic approach to the study of evolution. It was adopted, by this reviewer among others, as a text in university courses.

Praise for Mayr's 1942 book was not unalloyed with criticism. The most frequent adverse comments were those of non-ornithologists, who felt that Mayr relied far too heavily on data from birds; that his generalizations from ornithological data were not necessarily applicable to animals of other groups; and that his examples from non-ornithological sources were not always wisely chosen or correctly interpreted (partly based on personal conversations, but see Hubbs, 1943, and Schmidt, 1943). A more recent critique (Blackwelder, 1962) takes sharp issue with many of Mayr's viewpoints as expressed both in the 1942 book and in later writings.

Whether as a result of these criticisms or as a natural broadening as a biologist which would have taken place in any event, Mayr's interests in the years following 1942, as illustrated by his publications, seem to have expanded greatly. Among his papers we find titles dealing with such diverse topics as genetics and behavior of *Drosophila*, the taxonomy of fossil hominids, and speciation in echinoids. Until he left the American Museum of Natural History for Harvard in 1953, the majority of his papers still dealt with birds, and he wrote two highly useful regional bird guides (one coauthored with Jean Delacour). Increasingly, however, one notes in Mayr's bibliography the appearance of interpretive and synthetic papers. Since 1953, his publications have been overwhelmingly of this nature; scarcely a symposium has been published in the past ten years on evolution, classification, the "species problem," etc., that does not have Mayr listed as coeditor, participating author, or summarizer. His name continued to appear occasionally in the ornithological literature of the last decade, particularly in connection with technical details of nomenclature, and he served as coeditor for volumes 9, 10, and 15 of the Peters' "Check-list of Birds of the World."

All this while we had heard rumors, first of a revised edition of "Systematics and the Origin of Species," and later of a completely new book rather than a rewritten version of the 1942 work. The rumors are rumors no longer, and "Animal Species and Evolution" is now before us. It is, indeed, a completely new book, and more than twice as long as "Systematics and the Origin of Species."

The present review is appearing rather late, and I do not pretend to have avoided reading other reviews to prevent my being influenced by the opinions of others (although I have not yet, at this writing, read a review by an ornithologist). I have, in fact, eagerly soaked up such opinions. This procedure is virtually mandatory because of the overwhelmingly broad spectrum of Dr. Mayr's intellect, and of his book. Nobody is truly capable of a thoroughly analytical review of Ernst Mayr's book *in toto* except another Ernst Mayr, and such reviewers are rare indeed! In practice, Dr. Mayr's book *can* be reviewed at any of three different levels. The dust jacket bears excerpts from statements by eight world famous biologists. using such terms as "landmark," "definitive," "indispensable," etc. This may be called the "forest" level of reviewing, and some of the post-publication reviews in journals have also been at this level. The latter, however, tend to be at the "trees" level; the reviewers have expressed their admiration of the book as a whole, especially of areas lying outside their own fields of interest. Specialists reading Mayr's book tend to reflect the viewpoint of Gerald W. Johnson writing on I. F. Stone: "He has . . . the merit of tremendous industry. How the man covers so much ground and reads so much dull stuff is beyond my comprehension; but I respect it" (Johnson, 1963). Having acknowledged Mayr's broad coverage, the specialist then goes on to question rather critically Mayr's limning of those trees in the vast forest with which he, the specialist, is best acquainted. This is only to be expected. Loren Eiseley, in response to a criticism of his review of "The Columbia Encyclopedia," wrote ". . . in judging anything so extensive as an encyclopedia, one can only test the accuracy of detail by the examination of areas in which one has some reasonable degree of knowledge" (Eiseley, 1963). For most of us, attempting to review Dr. Mayr's book is not unlike essaying a review of an encyclopedia, save only that the latter is usually the product of many authors rather than one.

If one allows one's impression of "Animal Species and Evolution" to be formed from a synthesis of the criticisms of individual trees and groves, one may at least be permitted some doubts as to the soundness (in this case, the authoritativeness) of the forest as a whole. Given the immense scope of the book, however, this composite impression based on a mosaic of specialists' displeasure with Mayr's treatment of their pet subjects might seem to be somewhat unfair to the author.

There is one more level of reviewing which, to continue the sylvan metaphor, may be called the "twig" level. This involves the scrutiny of details of fact, citations of literature, use of scientific names, etc. Few reviewers have bothered to descend to this level, perhaps for lack of time, perhaps because of a dislike of being thought petty. One commentary which was presented at the "twig" level was that of Alexander (1963), which was answered (I daresay not wholly to Alexander's satisfaction) by Mayr (1963). Dr. Mayr began his reply with the following words: "No one can write a book of 813 pages with 1,800 literature references and numerous generic and specific names quoted on almost every page and not expect to make an occasional mistake. However, I hope that matters are not quite as bad as Dr. Alexander would seem to make them." Let us see. The forest has been adequately covered by reviewers, and specialists of various sorts have had their say about the trees. and a few have examined some twigs. In most of the present review, I shall be writing primarily as a specialist in ornithology, which was Dr. Mayr's original field. I shall pay a good deal of attention to twigs, in the face of a certain amount of unpopularity of this type of reviewing. But, as I shall mention in concluding, I think there is a need for this close examination.

In a synthetic work of this nature, the references to the literature are all-important (as suggested by Dr. Mayr's stressing of the fact that his book contains some 1,800). Checking such references is a tedious editorial task, but a vital one. Reviewers ordinarily expect that this task has been done, and will look up only such references as may catch their eye, either because of an apparent discrepancy, or a wish, unrelated to reviewing *per se*, to learn more about the subject. This is true of all of the literature citations mentioned below; I made no "spot-checks" for accuracy, but looked up only those references which interested me particularly for some reason.

On p. 94 of Mayr's book, in a discussion of seasonal isolation as an isolating mechanism, I encountered the sentence "The five species of *Rana* in eastern North America (Moore 1949) likewise have largely overlapping breeding seasons." Now, even as an ornithologist I know that there are more than five species of frogs of the genus *Rana* in eastern North America; I was certain that what Dr. Mayr meant to say was either "five *of the* species of *Rana* . . ." or "the five species . . . *studied by Moore*." So I checked Mayr's bibliography for "Moore 1949." The only reference under that date is a paper on geographic variation of adaptive characters in the leopard frog, which proved upon reading to have nothing to do with the subject in connection with which Mayr cites "Moore 1949." Dr. Moore does, however, mention in a footnote (Moore, 1949*a*:22) that more of his material on the genus *Rana* is to appear in a symposium volume "to be published in the near future by the Princeton University Press." The paper thus referred to (Moore, 1949*b*) turns out to be the one in which appear the data given by Mayr on breeding seasons of *Rana* (of which, incidentally, Moore mentions no less than twelve species in eastern North America in all), but this paper is *not* listed in Mayr's bibliography, although ironically enough it appeared in a volume of which Mayr was a coeditor.

Other inaccuracies involving literature citations may be mentioned more briefly. On p. 153 there are two references to "Dunn, in Mayr 1944." The only "Mayr 1944" in the bibliography is "The birds of Timor and Sumba," in which Dr. Dunn did not take part; the Dunn reference is alphabetized under that author's name without any mention of Mayr (actually Dunn's paper was a sort of appendix to one by Mayr which is not listed). Mayr relatively seldom gives exact page citations, even for short passages from long books; this in itself is an inconvenience. On p. 310, however, there is a citation of "Grinnell 1926:260." The only Grinnell 1926 listed in the bibliography has pages running from 429 through 450; the only Grinnell reference which *has* a p. 260 has nothing on that page remotely pertinent to Mayr's point. As documentation for a statement that ". . . many workers in recent years have attempted to calculate the average amount of dispersal per individual per generation . . ." (p. 566), Mayr cites among others a paper by A. H. Miller in which I am unable to find any such calculation. On p. 511 Mayr states that circular overlaps "have been shown to be probable for three species of ducks and geese in the Perry River region of arctic North America (Gavin 1947)." Gavin gives such evidence for two geese, *Branta bernicla* and *Anser albifrons*, but none for any species of duck. In some instances Mayr may state as fact what the author in the reference cited presented only as tentative conclusions, an especially dangerous procedure if these tentative conclusions are later shown to be incorrect. For example, Mayr (p. 511) gives a list of species in which "circular overlaps have been described," and includes without comment "*Charadrius hiaticula* (Bock 1959a)." In actuality Bock merely suggested that there might be such a circular overlap in *Charadrius*, and admitted frankly that there was no real evidence for it. Subsequently Vaurie (1964:2-4) has shown that it probably does *not* exist.

Having found, in areas of my special interests, such inaccuracies of citation and of second-hand presentation of material, and having read the comments of Alexander (1963) and of Brown (1964), I cannot help wondering to what extent I can rely on Mayr's citation of primary literature not readily available to me for verification.

Turning from bibliographic citations to matters more strictly ornithological, one again encounters disquieting passages, either having to do with matters of fact or of interpretation. Mayr's familiarity with the literature and the taxonomy of North American birds does not appear to be up to the standards of his knowledge of birds of the Pacific. On p. 117 he discusses what he designates "the so-called 'Potomac Warbler' " [i.e., *Dendroica potomac* Haller]. This possible hybrid may be "so-called" somewhere in the literature, but every reference I have ever seen and every ornithologist with whom I have discussed these enigmatic birds used the English name proposed by the describer, "Sutton's Warbler." In an additional reference to this presumed hybrid, Mayr states (p. 127) that it "comes from an area where the Parula Warbler (*Parula americana*), one one of the parental species, is rare." In point of fact, the Parula Warbler was common in that area, and the *other* presumed parental species, the Yellow-throated Warbler (*Dendroica dominica*), had never been observed, as clearly stated in the original paper (Haller, 1940). And I am informed by ornithologists who know much more about wood warbler behavior than I do that there is no justification for Mayr's speculation that "pair formation was apparently facilitated by similarity in the nesting behavior of the two parental species" (p. 117).

In the same discussion of hybridization, Mayr makes the valid point that "many of the known hybrids of animal species are found at the margin of the normal geographic range of one of the two parental species, or even beyond it" (p. 127), but then goes on to use a most unfortunate example. He states "The 'Cincinnati Warbler,' which appears to be a hybrid between the Blue-wing [*sic*] Warbler (*Vermivora pinus*) and the Mourning Warbler (*Oporornis formosa* [*sic*; = *O. philadelphia*]), was found in an area south of the range of the Mourning Warbler." In the first place, the presumed parents of the probable hybrid described as the "Cincinnati Warbler" are the Blue-winged and the *Kentucky* Warbler, whose misspelled scientific name ("*formosa*" = *formosus*) Mayr used for the Mourning Warbler; both of these species breed in southern Ohio, contrary to the point Mayr was trying to illustrate in citing this hybrid. In the second place, the specimen in question was collected on 1 May, a date far too early in the spring for any conclusions to be drawn about ranges of presumed parents; on 1 May this individual could have been five or five hundred miles from its hatching place. Dr. Mayr may have confused the original "Cincinnati Warbler" with a second, somewhat similar presumed hybrid which was taken in Michigan on 28 May 1948, and which is thought to be a possible offspring of the Blue-winged and Mourning warblers although collected slightly south of the known breeding range of the latter species (see Langdon, 1880; McCamey, 1950).

Mayr's choice of examples from the family Parulidae seems to have been persistently unhappy. On p. 304 he states "Most migratory species of the North American warbler genus *Dendroica* are geographically invariable." If by "geographically invariable" he means, as I assume he does, that no subspecies are recognized, he is just barely correct by the standards of the current A.O.U. Check-list—12 monotypic species to 10 polytypic. But "geographically invariable" is a little strong if one considers that subspecies not currently admitted by the A.O.U. have been described in at least two (*nigrescens*, *striata*) of the "monotypic" species of *Dendroica*. In fact, Mayr's repeated reference to monotypy in Parulidae (see also p. 417) is misleading when it is remembered that several species considered monotypic in the A.O.U. Check-list in addition to those in *Dendroica* exhibit geographic variation of less than the degree currently invoked for subspecies (cf. *Parula americana, Limnothlypis swainsonii*).

Many of Mayr's generalizations will, of course, be accepted at face value (especially by students), as they are troublesome to check. Some, when investigated, prove to be weak or even baseless. For instance, on p. 568 Mayr states "Fruit- and nectar-feeding birds which have to follow shifting food supplies show greater dispersal and less subspeciation than the more sedentary insect eaters." Perhaps logical enough, but let us test this generalization. An ideal group, differing chiefly in feeding adaptations, consists of the primarily insectivorous Parulidae (wood warblers), the primarily frugivorus Thraupidae (tanagers), and the primarily nectar-feeding species currently assembled as the family Coerebidae (honeycreepers), although some authors believe this to be a composite group of derivatives from the Parulidae and Thraupidae respectively. According to Mayr's generalization, the Parulidae should have the most subspecies per species. Using, for convenience, the species and subspecies as listed by Hellmayr (1935, 1936), we find that the insectivorous Parulidae average 2.37 subspecies per species, and the frugivorous Thraupidae 2.49; the nectar-feeding Coerebidae, even after subtracting the bias caused by the 22 insular subspecies of *Coereba flaveola*, still average an even 3 subspecies per species. These figures are exactly the opposite of what Mayr has led us to expect.

Another somewhat dubious generalization is Mayr's comparison of migratory emberizids with migratory parulids in which he suggests that the large amount of geographic variability shown by the former may be related to the fact that they are "ground-living birds and perhaps more exposed to selection by predators and microclimates than are species living in tree tops, such as most Parulidae" (p. 418). But among the most migratory and the most geographically variable of the Parulidae are the Yellow Warbler (*Dendroica petechia*) and the Yellowthroat (*Geothlypis trichas*), neither of which can be characterized as a tree top bird, and both of which occupy habitats shared with emberizids.

On p. 335 Mayr discusses the nineteenth-century species concept, using as his example the Song Sparrow (*Melospiza melodia*) and related species. His point is that the western forms of Song Sparrow *insignis, rufina, gouldii,* and *fallax* were "described as 'species' because to their describers they seemed as different from each other as the four original species [i.e., the Fox, Song, Swamp and Lincoln's sparrows] of eastern North America." An interesting notion, but wholly unfair to the describers who were working within a primarily binomial system of classification. Of the four western forms listed, I have been able to check the original descriptions of *insignis, gouldii,* and *fallax*. These clearly show that the describers knew perfectly well that their new forms were Song Sparrows, allied to and even intergrading with the Eastern Song Sparrow; *gouldii* is even referred to in one place as "var. *gouldii*" by Baird, its author. Incidentally, Mayr departs from A.O.U. Check-list usage in employing the generic name *Passerella* rather than *Melospiza* for the Song Sparrow and its relatives, although recent students of New World emberizines tend to agree that if generic lumpings are to be made, *Zonotrichia* (and, indeed, *Junco*) cannot be excluded from the assemblage (Bond, 1956:188; Dickerman, 1961).

Others among Mayr's generalizations would be exceedingly difficult to challenge. I would be curious, for instance, to know who has gone to the trouble to do the detailed research necessary to support a statement like "not a single geographic race is known that is not also an ecological race" (p. 357).

Some additional ornithological details deserve comment. On p. 598 the word "Proavis" is used without any explanation. Mayr may believe it to be self-explanatory, but a student would not be likely to know that this is merely a convenient name for a hypothetical undiscovered stage in the transition from reptile to bird. In discussing geographic variation in proportions (pp. 304–305), the choice of tail/wing ratios in the drongo *Dicrurus hottentottus* as the sole example given was infelicitous, as the "tail" in measurements of birds actually constitutes the tail *feathers*, epidermal structures such as are separately discussed in Mayr's next paragraph. On p. 324, the generalization that birds from northern populations of migratory species normally have relatively longer wings than more southerly populations is contradicted by the map on p. 322 based on Salomonsen's data for *Charadrius hiaticula*. The caption for the figure on p. 591 reads "Geographic variation of bill function in the Hawaiian honey creeper *Hemignathus lucidus*," but drawing "A" portrays a different species, *H. obscurus*, as the remainder of the caption indicates. On p. 371 Mayr refers to several North American birds which demonstrate east-west pairs of populations now united by hybrid zones. Among such well known examples as the flickers, towhees, and Myrtle/Audubon's warblers, he lists "ruffed grouse (*Bonasa*)." I know of no such situation in the genus *Bonasa*; Mayr no doubt meant the Spruce/Franklin's Grouse (*Canachites*), the only North American grouse with such an east-west pair. On p. 377 he again invokes the flickers, this time as an example of great variability in a narrow allopatric hybrid belt. But this "belt" in the flickers, judging from specimens exhibiting introgression, may well be the broadest among North American birds. On p. 564 the Cattle Egret is said to have "colonized northern South America across the Atlantic around 1930 . . ." whereas this colonization took place at least fifty years earlier (Bond, 1956:12).

Some of Mayr's usages of scientific names of birds are difficult to interpret. "*P. lazuli*" for *Passerina amoena*, the Lazuli Bunting, is clearly a slip of the pen on p. 118. On p. 345 Mayr uses the generic name *Edolisoma*, although in the Peters' Check-list (Mayr and Greenway, 1960) he himself had "lumped" this genus with *Coracina*. On the other hand, his use on p. 117 of "*T.* [*ympanuchus*]" instead of *Pedioecetes* as the genus of the Sharp-tailed Grouse is equally clearly an expression of his conviction that the latter species ought to be considered congeneric with the Prairie Chicken. Revival of the old name Cardinalinae (p. 97) for the subfamily known to most readers as Richmondeninae may be startling, but apparently has some justification in the technicalities of nomenclature (although this had not been made "official" at the time of publication of Mayr's book). Less clear is Mayr's use of *Quiscalus* rather than *Cassidix* in citing the work of Selander and Giller on the Boat-tailed and Great-tailed grackles (p. 87); this could either be a slip of the pen or another implied advocation of generic "lumping." It remains highly questionable whether a textbook of this type is the proper place for taxonomic or nomenclatorial innovations, especially when unexplained, no matter how soundly based these changes may be (see my earlier comments on this subject; Parkes, 1958:102).

Several reviewers have taken issue with Mayr on certain of his statements of principles involving various aspects of evolution, some major, some minor. Lest it be said that my review concerns itself with nothing but misquoted references or misspelled scientific names, let us proceed to matters of wider significance. On p. 389 Mayr quotes favorably what he admits to be a broad generalization concerning the characteristics of central versus peripheral populations of a species. Among these characteristics he lists relatively high population density per unit area for central populations. This may often be true; but peripheral populations are frequently members of depauperate faunas and may reach extremely high population densities, presumably correlated with absence of competing species, or of predators, or both, a phenomenon well known to visitors to small islands (see Tompa, 1962, for a good example). Incidentally, the figure on p. 388 chosen to illustrate characters of peripheral versus central populations of the drongo *Dicrurus* *leucophaeus* suggests that in this case "peripheral" and "central" have been defined to suit the example.

In discussing geographically isolated populations, Mayr (pp. 366–367) states that their "isolation is never complete, since a certain amount of gene flow reaches even an isolated oceanic island (or else it could not have been colonized originally)." Leaving aside the possibility that such an oceanic island may have been colonized by a combination of fortuitous circumstances with an infinitely small likelihood of repetition, this discussion does not allow for the development or the strengthening of a barrier *after* a colonization has taken place, effectively preventing even the small amount of gene flow inherent in the fact of the original colonization. In his comparison of the potential for speciation in central versus peripheral populations, Mayr makes two statements (top of p. 527, top of p. 535) that I cannot interpret other than as directly contradictory to one another. And surely circular reasoning is involved in Mayr's claim (p. 491) that he has "shown" that the earliest immigrant birds from Asia to Australia and North America have evolved into new families and genera, later ones into new species and subspecies, and the most recent have not yet begun to speciate. After all, the chief (often the only) evidence for the relative antiquity of such immigrations is the degree to which they have become differentiated (see Parkes, 1959:425ff.).

Mayr states on p. 60 that "In continental areas without physical barriers the border of the species range indicates the line beyond which the species is no longer adapted, and the very existence of such borders is tangible proof of the limitations of this adaptation." Although one might hedge by quibbling over the definition of "adaptation," this sentence as it stands does not seem to me to allow sufficient leeway for the principle of competitive exclusion, which is clearly discussed by Mayr a few pages later.

The superspecies is an exceedingly useful concept, and many recent authors, including the reviewer, have employed it. In actual use, however, there is an inescapable subjective element inherent in the choice of forms considered to belong to one superspecies, even more so than at standard hierarchal levels of classification. It thus appears a bit dogmatic to state flatly that "There are 17 superspecies (13.6 percent) among the 135 species of Solomon Islands birds" (p. 499).

Some points on terminology may also be brought forward. Mayr has included a useful ten-page glossary, but coverage is uneven. I encountered several terms which a student may well have wished to have defined ("isogenic," p. 174; "transduction" and "heterokaryotic fusion," p. 181; "euryecous," p. 345), although Mayr felt that it was necessary to define "firefly." Rather more serious is the lack of any attempt to define either "evolution" or "phylogeny" (the latter is also absent from the index). That these two terms cannot be considered self-evident is shown by the recent and thoughtful discussion by Bock and von Wahlert (1963).

Mayr's writing is clear and readable, even when discussing difficult concepts, and merits high praise when contrasted with the dense prose often found in evolutionary literature. In two places the choice of words in translations from German could be improved. The German "Stoff" is rendered better in English as "substance" than as the cognate "stuff," which tends to be a colloquial word; "sex stuff" on p. 100 has an almost ludicrous sound. On p. 356, in translating Steinmann's terms for ecological races of the European trout, the names Lake Trout and Brook Trout might better have been put in quotation marks and uncapitalized, as these are the accepted English names of two very different species. The book is pleasingly printed, and is remarkably free from typographical errors. I found only one which seriously affects the sense of the text, and that has already been called to our attention by Dr. Mayr (in Stebbins, 1964, footnote 2); on p.

521, "The absence of drastic reduction in gene flow . . ." should read "The absence *or* drastic reduction. . . ."

A major departure from the kind of discussion of speciation found in Mayr's 1942 book is the final chapter of "Animal Species and Evolution," entitled "Man as a Biological Species" (there is no index entry for either "man" or "*Homo*" in the 1942 book). This is an odd conglomeration including descriptions of the major fossil hominids, discussion of the variations in living *Homo sapiens*, political and social implications of evolution, and speculations on man's future. This chapter, or portions of it, has already been reviewed by specialists (see, for example, Newcombe, 1963). Although I stated that I would review Mayr's book chiefly as an ornithologist, I am, after all, a member of the species being discussed in the final chapter, so I will undertake to offer critical comments on a few points mentioned therein.

There are some striking contradictions to be found in this chapter. To begin with a minor one, on p. 626 Mayr states that the fossil genus *Limnopithecus* "is related to the gibbons," but that *Pliopithecus* is "even closer to the modern gibbons." This suggests that it would be stretching matters a bit to *call* either of these genera gibbons, but on p. 627 Mayr characterizes *Limnopithecus* as an "unmistakable gibbon."

At the top of p. 647, Mayr states "to look for and speak of 'pure races' is sheer nonsense," but halfway down the same page he contrasts "Human populations that are clearly the product of hybridization" with "unmixed races." On p. 656 Mayr states "none of these hybrid populations has produced an eminent person." The context does not make it clear whether he refers only to the specific populations cited several lines above (the Rehoboth Bastaards and the Pitcairn Islanders, neither of which one would expect to produce more than locally "eminent" persons), or to hybrids between major races of man in general. If the latter is meant, then the definition of "eminent" must be stringent indeed to exclude many historical and living persons of, for example, mixed Caucasian and Negro or Caucasian and American Indian ancestry.

There appears to be a discrepancy between the statement on p. 647 that some anthropoids and "many other animals" far exceed man in individual variability, and that on p. 648 which refers to the "high individuality of man." Although he does not actually employ a trinomial, Mayr's taxonomic discussion of Neanderthal Man (pp. 641–642) clearly indicates that he leans toward assignment of this problematical form as a subspecies of *Homo sapiens*. This is one of several solutions to the Neanderthal question under debate among anthropologists; I would question whether there are any other pairs of taxa of warm-blooded vertebrates which are currently regarded as subspecies and which differ as radically in osteological characters as do *sapiens* and *neanderthalensis*.

Mayr states flatly (p. 652) that the evolutionary trend toward increased brain size in hominids came to "a sudden halt" nearly 100,000 years ago, and postulates some factors to explain this "drastic reduction of the selective advantage of increased brain size." I have discussed this point with an anthropologist. In the first place, it may be a little premature to describe such an "abrupt halt" in talking about a period of less than 100,000 years (possibly substantially less, according to my friend), considering the order of magnitude of the time periods between the earlier stages of hominid evolution which demonstrate increase of brain size. But even granting Mayr's premise of the "abrupt halt," the factors he invokes in explanation are inadequate. These are an increase in the size of the "unit of selection" from the individual through the family to the tribe or nation; "The larger such a unit is, the relatively less will the genes of its leader contribute to the gene pool of the next generation and the more protected (biologically) will be the average or below-average individual of the group." And Mayr goes on to emphasize the "dysgenic effect of urbanization and of density-dependent diseases," and "the development of cultural tradition and the steady improvement in means of communication," pointing out that the achievements of the superior individuals enable the inferior ones to make a living and to reproduce successfully. All no doubt true, but the factors invoked to explain a supposed abrupt shift of selective pressures some 100,000 years ago could scarcely date back more than ten thousand (more likely around six thousand) years.

In discussing the effects of cultural tradition on the evolution of man (p. 656), Mayr runs into a semantic problem when he states that "cultural tradition is not altogether absent elsewhere in the animal kingdom." In man, "tradition" involves *telling* things to other individuals as well as *showing* them, especially as regards events in the past. In the migration routes of birds cited by Mayr (as well as in learned behavior, say, milk-bottle opening by titmice), *showing* only is involved, and it is dubious whether such phenomena should be called "tradition." Dictionary definitions of the word place special emphasis on the word-of-mouth aspects of tradition.

Although Mayr states in his preface that he has deliberately taken unequivocal stands on controversial issues, some flat statements in the final chapter, as elsewhere in the book, may conceal the controversial nature of the subject matter. The statement on p. 654 that "A rise in frequency [of genes controlling metabolic disturbances characterizing genetic diseases] will have no drastic effect on the future of mankind as long as adequate medical facilities are available" seems overoptimistic after one has read the contrary opinion by Muller (to which Mayr, in all fairness, gives a citation on p. 655).

It is perhaps time now to step back from our scrutiny of twigs, and assess the significance of our findings. This review, already lengthy, by no means includes all of the points jotted down for possible correction or discussion during my reading of the book. Thus there are more twigs susceptible to critical comment, based on my particular knowledge, than a simple count from this review would indicate. And, as previously mentioned, non-ornithological specialists have also contributed twig-level reviews based on their own fields of interest. There is a really important principle involved here, which is faced whenever major works of synthesis are to be evaluated, no matter what the subject. In a review of a book on China, Lindsay (1964) wrote "No one of the errors is particularly important, but their cumulative effect destroys confidence in the book as a reference work." I might not express my ultimate evaluation of Mayr's book in these exact words; for one thing, it is much more than a "reference work." But it seems to me that the reviewers at the forest level who have heaped unrestricted praise upon Mayr's hook have done so on the basis of an assumption—an assumption they had every reason to believe was correct, but one that the tree and the twig reviewers have shown was, unfortunately, unjustified. This assumption was, in brief, that the well-earned high reputations of Ernst Mayr and of the Harvard University Press, respectively, would insure that what industrialists call "quality control" of the text and references would be impeccable. Nobody denies that this book is a major contribution to the literature of evolution. The lively discussions in the pages of several journals indicate that the book has already had the "heuristic" effect that Mayr, in his preface, hoped for, and every serious student of evolution will, if he can afford the twelve dollars, buy it or have his library buy it. But this brings me back to my major summarizing point. A student who buys a major book published by the Harvard University Press and written by Ernst Mayr (whether considered in the light of his personal scientific reputation or simply as Director of the Museum of Comparative Zoölogy) has a right to expect a level of accuracy of detail that he just does not get in "Animal Species and Evolution." This makes all the more unfortunate the publisher's statement on the dust-jacket flap: "In accordance with the author's feeling that the acquisition of new knowledge will require a new statement, rather than an emendation of a previous one, no substantive revisions of this volume are planned for future printings." The key word here appears to be "substantive"; the philosophy expressed by the whole statement seems to be that the extant body of knowledge in this field has been definitively presented in "Animal Species and Evolution." Whether or not this is true, and to what extent "substantive" revision might, after all, be desirable, can best be determined by the author and publisher in response to this and other reviews.

LITERATURE CITED

Alexander, R. D.

1963 Animal species, evolution, and geographic isolation. *Syst. Zool.*, 12:202–204.

Blackwelder, R. E.

1962 Animal taxonomy and the new systematics. *In* Survey of Biol. Progress, vol. 4. New York, Academic Press: 1–57.

Bock, W. J. and G. von Walhert

1963 Two evolutionary theories—a discussion. *Brit. J. Philos. Sci.*, 14:140–146.

Bond, J.

1956 Check-list of birds of the West Indies. Philadelphia, Academy of Natural Sciences.

Brown, W. L., Jr.

1964 Two evolutionary terms. *Syst. Zool.*, 13:50–52.

Dickerman, R. W.

1961 Hybrids among the fringillid genera *Junco-Zonotrichia* and *Melospiza*. *Auk*, 78:627–632.

Eiseley, L.

1963 [Letter to editor]. *N. Y. Times Book Rev.*, 17 November:64.

Haller, K. W.

1940 A new wood warbler from West Virginia. *Cardinal*, 5:49–52.

Hellmayr, C. E.

1935 Catalogue of birds of the Americas and the adjacent islands, part 8. *Field Mus. Nat. Hist. Zool. Ser.*, 13.

1936 *Ibid.*, part 9.

Hubbs, C. L.

1943 [Review of Mayr, 1942]. *Amer. Nat.*, 77:173–178.

Johnson, G. W.

1963 Gadflying with I. F. Stone. *New Republic*, 149, no. 24, 14 December:21–22.

Langdon, F. W.

1880 Description of a new warbler of the genus Helminthophaga. *Bull. Nuttall Orn. Cl.*, 5:208–210.

Lindsay, M.

1964 [Review of] Twentieth Century China, by O. E. Clubb. *Science*, 143:1158–1159.

Mayr, E.

1940 Speciation phenomena in birds. *Amer. Nat.*, 74:249–278.

1941 The origin and the history of the bird fauna of Polynesia. *Proc. Sixth Pac. Sci. Congr. (1939)*, 4:197–216.

1942 Systematics and the origin of species from the viewpoint of a zoologist. New York, Columbia University Press.

1963 Reply to criticism by R. D. Alexander. *Syst. Zool.*, 12:204–206.

MAYR, E., AND J. C. GREENWAY, JR. (EDS.)
1960 Check-list of birds of the world, vol. 9. Cambridge, Mass., Museum of Comparative Zoölogy.

MCCAMEY, F.
1950 A puzzling hybrid warbler from Michigan. *Jack-pine Warbler*, 28:67–72.

MOORE, J. A.
1949*a* Geographic variation of adaptive characters in *Rana pipiens* Schreber. *Evolution*, 3:1–24.
1949*b* Patterns of evolution in the genus *Rana*. *In* Genetics, paleontology and evolution, ed. by G. L. Jepsen, E. Mayr, and G. G. Simpson. Princeton Univ. Press: 315–338.

NEWCOMBE, H. B.
1963 Intelligence and genetic trends [letter to editor]. *Science*, 141:1104–1109.

PARKES, K. C.
1958 [Review of] Vertebrates of the United States, by W. F. Blair *et al. Wilson Bull.*, 70:101–104.
1959 The palaearctic element in the New World avifauna. *In* Zoogeography, Am. Assoc. Adv. Sci. Publ. 51, "1958":421–432.

SCHMIDT, K.
1943 [Review of Mayr, 1942]. *Copeia*, 1943:198–199.

STEBBINS, G. L.
1964 The evolution of animal species. *Evolution*, 18:134–137.

TOMPA, F. S.
1962 Territorial behavior: the main controlling factor of a local Song Sparrow population. *Auk*, 79:687–697.

VAURIE, C.
1964 Systematic notes on palearctic birds. No. 53. Charadriidae: the genera *Charadrius* and *Pluvialis*. *Am. Mus. Novit.*, no. 2177:22 pp.